

Edward L. Tatum

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Interview with Edward Lorie Tatum, <sup>all</sup> IGN 1958 winner in Medicine and Physiology. He shared the prize with George W. Beadle (?) and the other half of the prize was given to Joshua Lederberg.

Q: The project is to find out what the different types of collaborations are, and what sorts of functions they serve. As part of that, whether eminent scientists work together in different kinds of ways from rank-and-file people.

The reason I think this might be important is that many of the people ~~x~~ who ~~right~~ write about teamwork think that eminent scientists behave very differently. I'm just trying to find out if it's so.

Can we start at the beginning, with the work you did with E. B. Fred and Peterson at Wisconsin when you were a graduate student. What sort of work did you do?

RATUM: This was my ~~XXXXXXXXXXXXXXXXXXXX/XXXXXX/XXXXXXXXXXXXXXXXXXXX~~  
graduate work in <sup>Graduate</sup> ~~medial~~ school, my dissertation ~~which~~ research, so  
they were my faculty advisors

Q: And Peterson was visiting Wisconsin at the time?

TATUM: No, he was professor of chemistry, and Fred was chairman of the bacteriology department.

Q: I notice you published a paper with them, on

thiamin as a growth factor in bacterium. Was this the topic of your dissertation?

TATUM: No, The ~~mt~~ main area was concerned with bacterial growth factors, growth factors for microorganisms. This was one of the factors that ~~we~~ were able to identify, so this was one of the isolated projects.

Q: Did the three of you work together on this particular project, or . . .

TATUM: No, As practically always holds true in graduate work, the major professors direct the research, give advice, but do not ~~participate~~ participate in the setting up of experiments. So it depends on what type of collaboration you're talking ~~about~~ about.

Q: In this particular case, when you were pretty much of a student . . .?

TATUM: Yes.

Q: When you say that they directed," did you go to them with problems that you ran into and hope that they would be able to solve them?

TATUM: No. As far as I can remember -- it was a good many years ago -- they would advise in the general orientation and selection of problems, many of which were offshoots of work that they had done or some previous students had done, and give some suggestions as to how to <sup>get</sup> start<sup>ed</sup> <sup>on a problem</sup>. Then the students would go ahead and work it out, with consultations and discussions with other graduate students -- senior graduate students, assistants, and so forth, and between themselves -- and once a week, or something like this, a major professor would ~~xxx~~ come through and check up and ask to see the data on what had been <sup>found</sup> during the week, and so forth.

Q: I notice that you were the senior author ~~x~~ of that paper.

TATUM: This is ~~xxx~~ purely customary. In most cases -- and this still holds in most laboratories -- the senior author is the one who does the work, the major portion of the work, and in decreasing order of direct involvement in the work, the rest of the names come.

Q: Is that the practice here at Rockefeller?

TATUM: It's ~~the~~ practice almost everywhere, with a certain few notable exceptions where a major professor may insist on having his name as senior author on all the papers out of the department.

Q: Who would decide on the cut-off point in this "decreasing order of involvement"?

TATUM: I suppose, in practice, there are a lot of criteria -- direct experimentation, manipulation in the laboratory, the degree of contribution of ideas, degree of contributions of facilities, funds, etc. -- the conglomeration averaging out of all of these kinds of things.

Q: Is alphabetical authorship used at all in biochemistry?

TATUM: Never that I've heard of.

Q: Well, we'll get along to Utrecht <sup>in</sup> and Holland. What did you do with Kogel?

TATUM: Kogel was at that time receiving quite a lot of attention because of his work with very active growth factors -- <sup>auxin</sup> auxin, a plant hormone ~~etc~~ which he ~~had~~ and his associates had isolated and identified, and also the profactor ~~which~~ <sup>which was</sup> vitamin, <sup>named</sup> biotin, a yeast profactor.

It was his success in the isolation and identification, as an organic-structural chemist, which led me to be interested in learning some of these techniques and applying them to problems which I was then interested in.

Q: Did you work on the problems that were part of his

research, or did you work on a project which you had chosen?

TATUM: I, we decided on a project there after I had arrived. It ~~xx~~ involved the nutritional requirements of the hemolytic streptococci, <sup>we</sup> to try to isolate and identify the ingredients in the medium that were necessary to these organisms. We didn't get very far with this, in part because that many years ago a number of the vitamins had still not been identified. It was too complex a picture. It was a multiple requirement.

Q: You wrote that Niel Fries was working in the laboratory at the same time. Did you two <sup>get work</sup> ~~work~~ together at all? ~~{XXXXXXXXXXXX~~  
~~XXXXXXXXXXXXXXXXXXXX~~

TATUM: ~~XX~~  
You did your homework very well.

Yes, he happened to be <sup>right in the same laboratory</sup> ~~out there at the same time.~~ I was well

~~[rest of this answer illegible]~~  
acquainted with him and part of the exposure to discussion with him, exposure to his interests eventually led to our interest in chromosomes (2)

Q: I wondered whether it was because you had worked with him that you wrote to him to find out what the nutritional needs of neurospora are.

TATUM: Well, as a matter of fact, ~~he~~ <sup>I</sup> had ~~written him~~ <sup>had... he had given</sup> me a number of publications, including his dissertation. All the stuff was in there so I didn't have to write to him

Q: Oh, I see.

TATUM: Yes, this is <sup>where the</sup> ~~the~~ so-called Fries medium, <sup>came from</sup> which we used to use, a composition.

Q: Beadle mentioned that you didn't know what the <sup>requirements</sup> nutritional ~~medium~~ would be, and you got to work on it and discovered that biotin was a requirement. Was that an early guess, <sup>that</sup> and was it influenced by the fact that you had been in contact with <sup>(Fries)</sup> ~~--?~~ ~~(Stamper)~~

TATUM: No, not particularly. Biotin, by that time, had been isolated in fairly pure preparations; shortly after it was synthesized. So it was available, the ingredients of the yeast extract, ~~and~~ natural materials. So when we started building up a synthetic medium - a pure medium - it was one of the materials we added. You see, it was a required material

material, which is turned out to be.

Q: What sort of person was Kögel?

A rather typical...

TATUM: I found him to be a very friendly person, a very capable person, very typical of the old German geheimrat. He was German. At that time he'd been in Holland, three, or <sup>four</sup> years. He ran a <sup>very</sup> strictly + compartmented laboratory, with every one's project completely separate. In general, we weren't supposed to discuss our subject with anybody else, including people in the laboratory -- only with him. It was a very strange atmosphere, having come from the United States, but rather typical of the old German geheimrat professor.

Q: What was the rationale behind that?

TATUM: ~~XXXXXXXXXXXXXXXXXXXX~~ I have no idea. Custom?

Tradition? An element of secrecy? ~~XXXXXXXXXXXXXXXX~~ This in part, I suppose, was sponsored in Germany <sup>by the very great competition</sup> during the ~~early days of the~~ <sup>there.</sup> ~~partition.~~

Q: Did you publish any papers with him?

TATUM: No.

Q: That's a fascinating notion. In the United States, people are not so secretive, are they?

TATUM: They're not... they're getting a little bit  
again its a function  
more so, ~~because~~ of the increasing competition for ideas which  
stems from the competition for funds. This depends -- to a  
large extent, on the individual characteristics of the person.

Q: Is the field you are working in now what they  
call a "hot" field?

TATUM: The field of genetics is; probably not the aspect of  
it that we're ~~now~~ directly involved with.

Q: I wondered whether the degree of secrecy has something  
do with that.

Not a direct function  
TATUM: It does. It's a function. <sup>^</sup> There are a lot of  
intervening factors  
~~such functions~~ tending to interact. It is and it isn't.

Q: And then you moved <sup>OUT</sup> down to Stanford to work with  
Beadle. Did you know him before?

TATUM: I never even heard of him until -- I guess <sup>the way it</sup> ~~it was~~  
happened was that he had a project involving  
~~Earl Hoffman~~ (and he had) had a project involving  
Drosophila and wanted some younger person who was  
familiar with biochemistry -- biochemically trained...  
familiar with natural products and their isolation



Wisconsin was the center of this earlier work. I guess he wrote to Peterson. So Peterson gave him my name when I was in Holland. The first thing I knew was that there was a cable or something like that. Beadle offered me this job.

Q: Was it common for a biochemist to work with a geneticist?

TATUM: Probably less so, in those days, than it is now. Now many geneticists <sup>have</sup> ~~seen~~ to be biochemists, too. I suppose this was not as common then <sup>There</sup> ~~it~~ <sup>less</sup> was just crossing over all sorts of boundaries.

Q: Beadle wrote that you stood ~~Did you stand~~ in the same relation to him as <sup>same</sup> ~~you had stood~~ in relation to <sup>Ephrussi</sup> ~~Peterson~~? And, because of that, I wondered whether this would --

TATUM: He might have been influenced by this type of cooperation, ~~just the way~~

Q: You were not at Stanford at the time <sup>Ephrussi</sup> ~~Peterson~~ was there?

TATUM: No. <sup>Ephrussi</sup> ~~Peterson~~ was at Cal Tech. I went to Stanford at the same time Beadle did. <sup>He notified me at</sup> ~~XXXXXXXXXX Then he went to~~ Utrecht.

Q: You started out on this work with Drosophila with him.

Was anyone else working on it? With the two of you?

TATUM: No

Q: You wrote that you were rather naive about modern genetics and problems of heredity at the time you went out there.

Did a kind of mutual education occur between the two of you?

> Tatum: Very definitely  
Was it hard to teach him biochemistry and <sup>for</sup> let him teach you . . . ?

TATUM: It was hard <sup>CST</sup> for him to teach me genetics.

Q: Why do you suppose that was?

TATUM: ~~HEHEHE~~ He'd been exposed to a greater extent to biochemistry than I had to genetics. Genetics was not <sup>in fashion</sup> a ~~function~~. It certainly was not a generally accepted area. It was not a popular area. Not even part of the standard curriculum. Still isn't ~~trxxxyxx~~ <sup>many</sup> in medical schools. ~~Except that~~ Most biologists, or many, <sup>biologists</sup> had a mere smattering of genetics in those days. So all I had, as I indicated in my paper, was a superficial experience in a course in evolution, which taught me comparative anatomy, skeletal evolution. There was not much about <sup>bio-</sup> chemistry. ~~in that~~

Q: Did the research on Drosophila proceed with the two of you planning each stage?

TATUM: Yes. Each ~~xxxxxxx~~ of us planned and was responsible for a manipulation of the Drosophila, injections that were necessary. He was concerned with the genetic aspects; I was mostly concerned with the biochemical. But we always planned the experiments for a number of years.

Q: How long did that work go on?

TATUM: Oh, it continued for maybe 3, 4 years.

Q: And were the two of you by yourselves?

TATUM: Well, except for a few graduate students that Beadle had at the time involved in other projects <sup>an</sup> assistant or two.

Q: Was the milieu of doing research very different, was it <sup>less</sup> ~~more~~ busy? From the way it is now.

TATUM: Well, science was not quite as competitive, <sup>not</sup> as high-powered as it is now. There were fewer people involved. The West Coast, at that time, was not nearly as busy, scientifically speaking. Stanford was a relatively small school <sup>so life was</sup> not nearly as much a graduate school. ~~I would say it was~~ considerably simpler, altogether.

Q: I know you two published quite a <sup>number of papers</sup> lot on *Drosophila*. Did you usually publish "Beadle and Tatum" or did you reverse it, "Tatum and Beadle" ~~sometimes~~?

TATUM: It was reversed, depending on whether the subject was primarily genetics. Then Beadle would be senior author. If it was primarily biochemistry, I would be.

Q: You also wrote that you did some work with Haagen-Smit . Was that at Stanford?

TATUM: No. This was long-range collaboration. Haagen-Smit had been a student <sup>+ colleague</sup> of K $\ddot{o}$ gel, working on oxin, when he came to Cal Tech. He was a very fine bio-organic chemist. When we needed some advice and some analyses, we called on him to cooperate. He stayed at Cal Tech and we shipped samples down to him. We consulted with him.

Q: Was this by mail, or did you two visit one another periodically?

TATUM: No, it was exclusively by mail.

Q: When did you switch to the *Neurospora* work?

TATUM: After about three years; around 1940. We didn't switch completely. It was a gradual transition/

to the other. The Neurospora work had built up; it got larger & larger and the Drosophila aspects smaller and smaller.

Q: I have a few names. <sup>of people who worked with you</sup> Frances Ryan came out ~~for~~ from Columbia. And Esther Zimmer <sup>was a research assistant for</sup> you people? <sup>(later Mrs Joshua Lederberg)</sup>

TATUM: Yes.

Q: What did ~~XXX~~ Ryan do when he was out there?

TATUM: Well, he was working a little on metabolism, general aspects of growth, comparison between different criteria of growth -- dry weight, that was <sup>accumulated</sup>; and growth on solid <sup>a</sup>medium -- distance growth. He was actually responsible for the invention of the so-called Growth tubes or Race tubes -- just a little bent pieces of glass -- inoculate one end and as the agar grows <sup>across</sup> down, you can measure the ~~weight progression plus~~ <sup>rate of</sup> a two-dimensional surface.

Q: As your project grew, what was its maximum size? How many people were involved?

TATUM: About the time that I left to go to Yale, there probably were ten or a dozen people.

Q: Were you and Beadle directing the project together, or were you responsible for the biochemical parts and he for the

genetic parts?

TATUM: Well, yes, in part. But many of the people who came in were more or less senior investigators, too. They took over certain problems, and they were on their own.

Q: And did these result in papers that <sup>all</sup> ~~a lot~~ of you . . . ?

TATUM: Not very often, ~~xxx~~ except for very general summary papers.

Q: With that number of people, did you ever have the problem of keeping in touch with what everybody was doing?

TATUM: No. We'd have regular get-togethers and discussions. It was not that large. We <sup>still talked to</sup> ~~kept track of~~ each other.

Q: You ~~may~~ have more people working with you here?

TATUM: No, about the same.

Q: What sort of work are you doing now?

TATUM: We're still working with Neurospora <sup>among other organisms</sup> and but more and more interested in questions of biochemistry and genetics and morphology, and differentiation, which must be

finally put on a biochemical basis. Factors that are involved in the organization of cells and their structure. This is one of the newer aspects, one of the least-known aspects of biology.

Q: Are you the director of this work or do you have several senior people?

TATUM: We have several senior people -- three, as a matter of fact. <sup>in the group</sup> Each has primary responsibility for certain aspects of the work, and we see one another regularly. A very loose chain of command in the of organization. (laughter)

Q: When did ~~XXXXXX~~ Lederberg come to you <sup>at Yale</sup> again?

TATUM: In '46, I think.

Q: Ryan was the one who sent him on to you?

TATUM: Yes. Ryan had been interested in Lederberg a little bit. He ~~XXXX~~ had done some work on Neurospora ~~XXXXXXXXXXXXXXXXXXXX~~ ~~XXXXXXXXXX~~ ~~XXXXXXXXXX~~ and he had done some work <sup>at</sup> ~~for~~ Columbia, as a medical student, he became interested in ~~indicating~~ the possibility of the recombination of bacteria, in which ~~had been~~ building stocks - mutant stocks and ~~t~~

Ledeberg wanted to get in on this, he had ideas. He wasn't quite as interested in medicine as he once had been. And he elected to take a fellowship to work with me at Yale where he got his Ph.D

Q: Did you <sup>spot</sup> ~~mark~~ him as a bright young man, right off?

TATUM: Oh, there's no question about that. He was a little bit of a disturbing influence in lab for a while.

Q: In what way?

TATUM: He was a very sloppy experimenter. He broke more equipment in six months than any graduate student I ever had. His mind outstripped his fingers.

Q: Did he stand in relation to you as you stood in relation to Peterson and Fred, or was he more active a collaborator?

TATUM: I'd say more active a collaborator. <sup>Put it the other way,</sup> I think I was a more active collaborator than ~~him~~ Peterson and Fred. They had a great many more additional responsibilities than I've got. Each of them ran a complete department, practically. At no time have I ever had more than ten or a dozen people with me.

Q: Do you prefer to have a small group?



TATUM: Rather small, yes.

Q: Have you ever had experience working with larger numbers -- 15 or 20?

TATUM: No, I've avoided that, religiously.

Q: Why is that?

TATUM: I just won't. You get removed so far from the actual experimental work when one has to split one's time among 20 or 30 people, and teach, and administrative responsibilities, and <sup>the rest</sup> ~~so forth~~. You'd have very little time for any one aspect of it. It's always seem to me that this would be a very unsatisfactory way of life for the particular type of thing that I am <sup>enjoy</sup> ~~doing~~ <sup>why</sup> ~~it~~ <sup>is</sup> ~~do not~~.

Q: So you've always arranged that you'd be able to have a hand in each thing that was going on.

TATUM: <sup>Yes, yes.</sup> Yes. I have tried to assure the maximum amount of time, but it always gets out into by committee meetings, and things of one kind or another, interviews... That was mean but I couldn't resist. You know I don't really mean it.

Q: Did you try to convince Lederberg not to go back to medical school?

TATUM: I didn't try to convince him one way or another. He didn't need any convincing that this was his decision.

Q: Do you feel that the work for which you were given the prize was the best piece of work that you did?

TATUM: Well, I wouldn't call it a "piece of work," as such. It involved a great many pieces. The <sup>general concept, yes</sup> ~~best contribution,~~ <sup>let's</sup> ~~say, conceptual~~ contribution. Yes. <sup>The</sup> Of either of us.

Q: Have you ever done any research that you consider pedestrian?

A great deal

TATUM: ^ One never can tell, when you start a piece of research, how it's going to turn out. <sup>Necessarily,</sup> ^ One has to gather a lot of facts, on phenomena. , and if they're not interpretable easily, then in a sense they are by definition pedestrian. But I do feel that most of the time there is a fundamental aspect in almost any type of work which, if it can be seen and recognized, would take it out of the pedestrian class.

Q: Aside from the work with Beadle, and later with Lederberg, have you ever worked with anyone else over a fairly long period of time?

TATUM: I've worked with most of the graduate students three or four years each. Two, in particular, were with me after their graduate work. One of them did his graduate work with me and continued on at Stanford -- Ray Barrett, now at Yale & at Stanford up at Dartmouth. I think we were together, altogether, about eight years. The second was a student of Frances Ryan's, Sam Gross. He came out to Stanford ~~as a~~ postdoctoral ~~stage~~ and was with me there for two or three years and then came on to serve at the institute for two or three years, <sup>6 years altogether.</sup> He's at Duke University.

Q: Do you find that the longer-term collaborations are more satisfactory in getting work done? <sup>than the short term...</sup>

TATUM: Well, it always becomes of more value. You get a better return, scientifically, on an investment, <sup>of time & energy</sup> as the time increases, as you get to know each other, <sup>knowing his point of view as problems develop.</sup> Three years is minimal.

Certainly, <sup>even</sup> one year, <sup>in</sup> two years you barely have time to get things started and outlined and theory developed and <sup>explore</sup> ~~expose~~ some possibilities before it's time to terminate.

Q: Aside from your work with K6gel, have you worked completely by yourself?

Very much with the people in my group.  
TATUM: There have been some isolated projects, in amphibious toxin and physiology, and certain aspects of micrometabolism, which for a time I was working on at the Marine Station; things of this type. But these are all isolated, single-shot <sup>type</sup> projects. collaborations. All of the continuous programs have been within our own group essentially.

Q: Have you ever worked completely alone, without anyone except perhaps a technician?

TATUM: I think <sup>as</sup> ~~that~~ most people, when you have 8, or 9, or 10 different projects, I think everyone likes to have one that they're playing around with themselves.

Q: Do you like working by yourself?

TATUM: Yes.

Q: Some people have said that one of the advantages of working alone is that you can work pretty much at your own pace. Have you found this?

TATUM: Yes, this is true. But it becomes more and more difficult to work with any continuity, relying only on one's self, <sup>the demands on one's</sup> <sup>is</sup> <sup>broken</sup> <sup>as</sup> other, ~~than the time~~ increase, the time/actually ~~grows~~ into. ~~fix~~ I essentially could do very little [interruption]

Q: Some physicists say, for example, that they could not possibly work by themselves in the kind of work that they do. In your field, has this become more and more <sup>the</sup> ~~of a~~ situation?

TATUM: It's probably more true in physics than in biology. I think the physicists have more of a need to hang around, <sup>an idea to</sup> ~~molding~~ and shaping, <sup>it to criticize</sup> than the biologists. There are areas in which it's <sup>valuable</sup> very ~~mixtix~~ to discuss projects, but it's not essential ~~is~~ for something ~~to be~~ worthwhile to be accomplished -- not to the same extent. It depends on the area.

Q: Well, what area of biology do you think would be closer to ~~ph~~ physics in this sense?

TATUM: Probably, in genetics, the area of coding, and mathematical analysis of certain <sup>rather</sup> ~~esoteric~~ aspects of ~~gx~~ gene function, where it becomes a problem of interpretation, which isn't clear-cut. <sup>In which</sup> There are lots of ~~gx~~ possibilities and the probable answer comes out essentially from the interstimulation of discussion of problems. <sup>where</sup> ~~When~~ there are so many factors to <sup>be</sup> <sup>ed</sup> consider, no one person can evaluate

all of them satisfactorily.

Q: Can you tell me whose judgment of your work matters to you most?

TATUM: I think my own.

Q: Are there other biochemists you send preprints to and talk about your work with?

TATUM: We talk about our work, but we do not send preprints.

Q: Is this a tradition in the field?

TATUM: It's a tradition of my <sup>lab.</sup> ~~own.~~

Q: Why is that?

19.1 TATUM: I just don't see the point of it. Any more than I see the point of insisting on the publication of the results of an experiment two weeks after it's been completed. If its good, it will keep. Nothing is so vital that it will make  
16.4 very much ~~big~~ difference to the ultimate progress of science whether it comes out <sup>now or</sup> ~~in~~ six or eight <sup>months from now.</sup> weeks. The emphasis on rapid publicatio

from the standpoint of the ~~experimental~~ theory which is expressed <sup>that</sup> ~~as~~ science, the progress of science, depends on the publication of this little, piddling -- or maybe ~~it's~~ not so piddling, <sup>It doesn't matter --</sup> this is the expressed opinion, justification for rapid publication of preprints & all the rest. I have the feeling that much of this is purely to establish priority in the a somewhat competitive field. Many ideas come to a lot of people at the same time. With the stimulation of some paper or some observation, everybody dives into it, and so they want to get credit, so they're in a hurry to publish. This is, I think, a <sup>purely</sup> selfish reason, which is the basic reason, for insisting on extremely rapid publication -- the <sup>question of</sup> priority, in contrast to the expressed excuse, that <sup>being for the advantage</sup> ~~in this is good for the progress of science~~. This is a cynical attitude, perhaps. For this reason, we do not publish in the biochemical "Quickies", nor do we send out preprints.

Q: DO you think that this emphasis on priority works to the detriment of the field?

TATUM: Definitely. People publish without having explored all of the possibilities; without being certain, sometimes, <sup>even</sup> of the facts. This adds to their publication lists. Three months <sup>later</sup> they can correct their errors & publish another paper.

Q: Have you ever been involved in a priority dispute of that kind?

TATUM: No, because we stay clear of the field which you spoke of a while ago as a very "hot" field, in as much as we all

Q: You stay clear because . . . ?

TATUM: We don't want to be involved in this competition. And if a field is this competitive, it will get done just as well whether any one individual is <sup>or not</sup> involved. I'd rather be taking more ~~whether any one individual is involved~~ of a gamble and going off on something that is not as sure-fire, not as receiving a lot of attention, and the returns are in a way somewhat more exciting because they're different .

Q: You mentioned before that in biochemistry the man who contributes most is given senior authorship. <sup>Do you usually notice</sup> ~~That is the usual~~ <sup>the</sup> sequence of authorship on papers you read?

TATUM: Oh, yes.



Q: Is it pretty clear who is responsible for what in your field?

TATUM: If you know the people.

Q: Do research assistants get authorship, ever? You probably don't have any, the way you did at Yale.

TATUM: We have technicians and we have doctoral and pre-doctoral students, research associates -- of course, they do.

Q: Do you ever find, in biochemistry, publication of a whole paper by a laboratory that will just say, "Rockefeller Institute" or . . . ?

TATUM: I've never seen a paper published by the Rockefeller Institute ~~without individual~~ ~~authorship~~.

Q: Do you find that a man gets ~~less~~ less recognition publishing with others than he would if he published by himself?

TATUM: No. It doesn't have any bearing on it. This is, in part, because it's the custom, because everyone recognizes what relative contributions <sup>are</sup> . As an illustration, for example, many fields in modern science need <sup>collaborative</sup> cooperative work -- whether in a laboratory or even between laboratories, or between different people in different institutions. It's a perfectly natural

development of the capacity of the science .

It should be so. There's still <sup>a place</sup> ~~plenty of work~~ for an individual, <sup>worker</sup> ~~but~~ working by himself. .

Q: <sup>Have</sup> ~~Do~~ you ever <sup>felt</sup> ~~think~~ that you've gotten undue rewards for the work you've done?

TATUM: <sup>You never think that</sup> No, I ~~haven't thought that~~.

Q: Have you ever felt surprised at the recognition that your work merited?

TATUM: No.

Q: Now, this may seem like a very odd question. If you had to choose between making a fundamental discovery that was anonymous, and one less significant for which you were given credit, which would you choose?

TATUM: <sup>Probably try to do</sup> Both of them.

Q: If you had to make a choice?

TATUM: I really couldn't <sup>predict</sup> ~~project~~.

Q: Have you ever had occasion to exclude your name from the authorship of a paper? For example, with graduate students or younger people who've been working with you?

TATUM: A few times. One piece of work was done jointly <sup>two</sup> by ~~my~~ graduate students, one in my group and one in another group. <sup>They were married.</sup> They couldn't decide just what to do. The students had wanted both their faculty advisors to be authors <sup>on the paper</sup> of ~~it~~. We didn't feel this was cricket, so we both withdrew our names from it. So the husband and wife published jointly.

This is not a question. It depends on the relationship, <sup>relative to the</sup> ~~mere~~ contribution to a particular problem. That's all.

Q: I have been told, particularly for senior people, that when they do publish jointly with younger ones, the younger ones are never credited with the work.

TATUM: I think this is not really fair. I mean, it's not a correct generalization. If I see a paper with three authors listed, the last author being one of my friends, I know that he has not done the work. <sup>in the laboratory</sup> He may <sup>or may not</sup> have contributed most or all of the ideas; he probably hasn't. He's stimulated <sup>it</sup> and made the atmosphere in which these are possible. But I may not remember the first author's name unless it is repeated, unless he continues <sup>to produce</sup>. If it's a one-shot deal, then one loses track of him and if he doesn't do anything else, well, then you remember the senior one. If he continues to <sup>publish</sup> ~~do~~ others, then he will shortly no longer be in association with

the elder men, and then he becomes established. So, to answer your question, <sup>the senior author who is the</sup> the junior person is sometimes lost sight of, but only temporarily, if he continues to produce.

And in many cases, he actually gains in acceptance of his work and general acceptance, by having had once ~~such~~ <sup>ed</sup> association with the senior person.

Q: I suppose it's more likely that the paper gets noticed. How is it decided what the order of the names will be? Is it discussed?

TATUM: It's discussed.

Q: Is there usually <sup>dis</sup>agreement? -- or is agreement more common?

TATUM: <sup>I've never known there to be disagreement</sup> There is usually agreement within our group. When we get into collaboration with another group, then there may be points of disagreement. But not necessarily affecting the people in our group. I can give you some examples without mentioning names. There was a specific side issue involving one person in another institution in this country and another person in a third institution. One of the other people wrote the paper and as

the senior person, age-wise and et cetera, insisted on having his name first on the paper. We objected, because the person who did all the work was the third person, ~~and~~ also not in this group. He was put second. So there is some disagreement; not as far as our group is concerned but as ~~far as~~ <sup>on the</sup> fairness of distribution. ~~Shouldn't~~ have been decided by somebody else. So it happens.

Q: You said before that you always wanted to keep your group small, so that you could have a chance to do the work you wanted to do. Have you ever had the sense that creativity gets restricted when large numbers of people work together? <sup>Because</sup> ~~all~~ of the planning involved?

TATUM: I don't think so. <sup>it becomes suppressed to some</sup> ~~Not to the extent, that~~ <sup>when</sup> the size, the bookkeeping, the mechanical aspects of too many technicians to keep busy and things of this kind - ~~rather than~~ administrative responsibilities - get to great.

As a function of group size? I don't think so. The type of people we like to have in our group are independent-minded. They may cooperate on project A and they may carry on project B and C on their own, or other types of collaborative sharing.

Q: There are complex relationships then?

Tatum: Oh yes, we have ample space and time for

individual work, original work.

Q: It's also been written that "team work" tends to be more mediocre than individual work. Does this make sense?

TATUM: I think I would put this in the same category. Individual work can vary, from mediocre to good. Teamwork can vary from mediocre to good. About the same distribution. There may be a little difference in the skew, skewness of the curve. This depends to such an extent on the individuals involved. I'm not sure one can generalize. I don't know how I could prove it one way or another.

Q: Have you ever been involved in a piece of work with other people that you wished you could have done by yourself?

TATUM: No, I don't think so.

XXXX Q: When you <sup>do</sup> ~~did~~ work with others, was there every any disagreement about how the work ought to be conducted?

TATUM: There may be a lot of viewpoints expressed, but <sup>this</sup> ~~how~~ is part of collaborative work. One discusses <sup>es</sup> ~~can you work without discussing~~ all possibilities and thrashing <sup>over-all approach</sup> ~~is~~ them out, and deciding <sup>pros and cons</sup> what the best or most reasonable <sup>way is</sup> ~~way is~~? This is <sup>the</sup> more a part of collaborative work than ~~this is actually part of~~ doing an experiment. It doesn't matter <sup>who puts reagent X into a</sup> ~~if individual X puts the entire test tube in~~ and individual B puts ~~the X and~~ Y in and individual C shakes it up. It's a collaboration of ideas rather than experimentations. <sup>that really</sup> ~~that really~~ count.

Q: In the field now, is there general agreement about what problems need to be solved <sup>or</sup> are the foci of research very varied?

TATUM: There's pretty general agreement ~~on~~ on the major goals, and rather fuzzy concepts <sup>on</sup> just how to reach those goals. We don't know what determines the structure of <sup>an organism</sup> ~~the work~~ -- whether it is organismic, cellular, or what. We're still exploring ~~as to~~ how best to approach this problem. How one really pinpoints it and gets down to brass tacks and gets definite experimental evidence one way or the other.

So this part of it is somewhat puzzling.

On the goals everyone agrees. We <sup>need to</sup> ~~must~~ know about the biochemistry of intracellular, organismic interrelations of cells. <sup>at the biochemical level. I can't answer anymore specifically</sup>

Q: You received the prize several years ago. Has it made a great difference in the way your work has proceeded? Have you had more access to funds than you had before?

TATUM: It's made very little difference except for being involved in more extracurricular activities.

Q: Do you feel a kind of responsibility to get involved in these extracurricular activities?

TATUM: To a certain extent, I always have. Opportunity increased.

Q: Do you think that this ~~business~~ business of how ~~at~~ the Prize affects your life depends on the age at which you win it? For example, <sup>Lederberg</sup> ~~the man who~~ was joint winner with you, have you noticed that his life has changed more than yours, for example?

TATUM: I would say it probably has.

Q: Do you keep in contact with Lederberg and Beadle?

TATUM: Not very closely. We all have our individual problems, lives, and <sup>projects</sup> ~~concepts~~, and responsibilities. Unless we happen to meet or have some reason for getting in touch, we don't.

Q: Do you get to see the other Laureates?

TATUM: Oh, they're always at scientific meetings.

Q: One other thing -- were you an only child?

TATUM: I have a brother and sister. I was the first.

Q: Do you think there's anything I've missed in trying to characterize the different sorts of collaboration.

Tatum: I think you've done pretty well.